giving in tabular form, so that it may be seen at a glance, some of the principal peculiarities of these 60 organisms, i. e., size, flagella, whether staining by Gram's method, aerobic or anaerobic, liquefaction of gelatin, growth in bouillon, growth in milk, spore formation, pigment on agar, formation of $H_2S$, indol reaction, amount of acid produced from grape sugar, gas production, growth in $CO_2$ and finally amount of growth in various media titrated as follows: (1) Neutral to phenolphthalein; (2) No. 1+10 cc. per litre of $\frac{N}{1}$ Na OH; (3) No. 1+10 cc. per litre of $\frac{N}{1}$ $H_2$ $SO_4$; (4) No. 1+20 cc. per litre of $\frac{N}{1}$ $H_2$ $SO_4$. Authors have used phenolphthalein for titrating media regularly since 1894 and recommend it for general use. "Jedenfalls kann der mittelst Phenolphthalein neutral hergestellte Nährboden unbedingt als Universalnährboden empfohlen werden." All the bacteria figured in the Atlas were grown on media slightly alkaline to phenolphthalein, and most of the 60 sorts bore the extra 10 cc. of alkali and the 10 and 20 cc. of acid. This seems rather surprising to the writer and certainly cannot be assumed to hold good for all species. My experience would lead me to select for a universal medium a grade of alkalinity considerably less than the zero or neutral point of phenolphthalein, i. e., one nearer the zero of the best neutral litmus paper, as I am satisfied that some species will not grow on media as alkaline as here recommended. In conclusion this book may also be commended to the physician and general reader who wishes to know something about bacteria without becoming swamped in details. Its remarkably low price (15 marks) puts it within the reach of everybody.—ERWIN F. SMITH.

**Science Sketches.**[2]—This small book of twelve reprints needs little comment. Those who read the sketches in *Popular Science Monthly* and elsewhere will doubtless desire to have them collected into one volume. It may be noted that the papers "Agassiz at Penekese," "The Fate of Iciodorum," "The Story of a Strange Land" and "How the Trout came to California" have taken the place of certain others in the first edition.—F. C. K.

**Recent Papers Relating to Vertebrate Paleontology.**[3]—The first paper below cited is a review by Dr. Baur, of Chicago, of a

[2] David Star Jordan, 2d Ed. A. C. McClurg & Co., Chicago, $1.50.

[3] Bemerkungen über die Phylogenie der Schildkröten, von G. Baur, Anatom. Anzeiger, XII, 24-25, 1896, p. 561. Jena.

On the Morphology of the Skull of the Pelycosauria and the Origin of the Mammals, by G. Baur and E. C. Case; Anatomischer Anzeiger, XIII, u. 4 & 5, 1897, p. 109. Jena.

paper by Van Bemmelen on the Phylogeny of Tortoises read before the Zoological Congress of Leyden. In this review Baur shows that Van Bemmelen has fallen into a good many errors of interpretation based on embryologic grounds, and presents a sketch of what is no doubt the correct phylogeny of the order Testudinata. The two papers constitute an excellent commentary on the necessity of interpreting embryologic data by the facts of paleontology. An appendix discusses briefly the characters of the Otocœlid family of the Cotylosauria, which the reviewer has regarded as the Permian ancestor of the tortoises (Proc. Amer. Philos. Soc., 1896, p. 122). Baur does not consider this proposition to be proven. He observes that the element which I have called clavicle includes both clavicle and cleithrum, but produces no evidence to support such a view. Were Otocœlus a Stegocephal, his idea might be probable, although the cleithrum is not distinctly visible in the Stegocephal Eryops; but as the former genus is a Cotylosaurian, i. e., a reptile, it is highly improbable, as no reptile is known to possess this element. He also remarks that the possession of a carapace means "gar nichts" in this connection. When, however, we read (p. 557) that "the characteristic of the tortoises is the carapace" it is evident that the words "gar nichts" are much too emphatic. Indeed the possession of a carapace is the essential of an ancestor of the Testudinata, since the Triassic forms possess one already well developed, as Baur has the merit of showing.

In the second paper Dr. Baur in connection with Mr. E. C. Case, describes the best preserved skull of Dimetrodon yet obtained. The authors add some important points to the osteology of the Pelycosaurian skull, but curiously enough do not refer to the anticipation of many of their results in the description and figure of the nearly allied genus Naosaurus published by the reviewer in the year 1892 (Trans. Amer. Philos. Soc., p. 14, pl. II, figs. 7, 7a). They add to what is there stated the description of the bones of the preorbital region, and determine the entire distinctness of the supramastoid ("squamosal")

Ueber den Wirbelbau b. d. Reptilien u. e. a. Wirbelthieren, von A. Götte; Zeitschrift f. Wissensch. Zoologie, LXII, 3, 1896. Leipzic.

Psittacotherium, a Member of a New and Primitive Suborder of Edentata, by Dr. J. L. Wortman. From the Bulletin of the Amer. Mus. Nat. History New York, Nov., 1896, p. 259. The Ganodonta and their relation to the Edentata, by J. L. Wortman, M. D., loc. cit. pp. 59–1896.

The Stylinodontia, a Suborder of Eocene Edentates, by O. C. Marsh, Amer. Journ. Sci. Arts, 1897, Feb., p. 137. New Haven.

Contributions from the Zoological Laboratory of the University of Pennsylvania, No. VII.

element. They announce the presence of a supramastoid arch whose elements were shown to exist in Naosaurus in the paper above cited. They, however, show what I did not discover, that it is separated by a foramen from the postorbitosquamosal arch. This foramen is either not present in Naosaurus, or it has been closed by pressure in the specimen I described. They describe the palatal structure better than has been done hitherto, which turns out to be quite similar to that which I had shown to exist in the contemporary Cotylosaurian genus Pariotichus. It is important to notice here that the supramastoid is identified with the bone called by Baur in the Lacertilia the squamosal. This identification may well be questioned, since it is purely a roof bone in these paleozoic reptiles, while I have shown that Baur's squamosal enters into profound articulation with the cranial walls in the Mesozoic Pythonomorpha. And it is the latter that must explain the nature of Baur's "squamosal" in the Lacertilia, and not the more remote Paleozoic types. (See my discussion of this subject, AMERICAN NATURALIST, 1895, 855, 1003). But whatever the relations between these elements, neither is the squamosal of the Mammalia, which I can now show is the element which I have sometimes called supratemporal and which Baur calls prosquamosal.

As a phylogenetic inference they assert that the Pelycosauria cannot be arranged with the Anomodontia as a suborder of an order of Theromora, because in the Anomodontia there is only one postorbital bar. This, according to my definitions, is true, but supposing, that the Pelycosauria cannot be arranged with the Anomodontia on this ground, the statement that the Theromora "do not exist," is not justified. In 1869[4] the reviewer revised this order, and included in it the Placodontia, Proganosauria, Parasuchia, Anomodontia, Pelycosauria and Cotylosauria. In 1891[5] it was further revised by the inclusion of the Proterosauridæ. In 1894,[6] following the statements of Lydekker, that the Proganosauria (founded on Stereosternum and Mesosaurus only) is probably a Sauropterygian type, this group was omitted, and the Procolophonina of Seeley was inserted, the Cotylosauria and Pseudosuchia having been already eliminated. The name Proterosauria (Seeley) was retained to represent the suborder for which I had used the name Proganosauria, minus the Mesosauridæ (type of Proganosauria). The order thus constituted included the Placodontia, Proterosauria, Anomodontia, Theriodonta and Pelycosauria. I now add that it is probable that the groups discovered by Seeley in S. Africa called

[4] AMERICAN NATURALIST, October.

[5] Syllabus of Lectures on Vert. Paleontology, July, p. 37.

[6] Proceeds. Amer. Philos. Society, p. 110.

by him Gomphodontia and Cynodontia (which are, perhaps, not distinct from each other as suborders) belong to the Theromora. They coincide, in all important points, differing chiefly in dentition, a character in which the Theromora present as many types as the Marsupialia.

In view of these facts it became the duty of the authors of the present paper to retain the order Theromora, so long as others had preceded them in reconstructing it with the advance of discovery. Also in discussing the phylogeny the authors should do their predecessors the justice to quote their latest opinions, and not their earliest, which they had modified or abandoned. In the paper above cited,[7] and others, I advanced the hypothesis that the Mammalia were derived not from the suborder Pelycosauria, as I had at one time supposed (as cited by Baur and Case), but from the more comprehensive order Theromora, a conclusion to which they do not refer. In one paper[8] I remark, "The Pelycosauria *could not*, however, have given origin to the Prototheria, since in that class of mammals there is a well developed coracoid," etc.

The phylogenetic inferences of the authors may be learned from the following quotations. After citing my opinion of 1884 that "the mammalia are descendents of the Pelycosauria," they remark (p. 118) "It is quite evident that the Pelycosauria with the two temporal arches and the specialized neural spines cannot be the ancestors of the Mammalia; they represent a specialized side branch of a line leading from the Proganosauria to the Rhynchocephalia, which becomes extinct in the Permian." It must be remarked here that the specialized neural spines are not a character of the Pelycosauria, as some of the genera do not possess them; and I never introduced them into the diagnosis. The case is similar to that of the basilisks which have enormously elongate neural spines, yet the genus is one of the family Iguanidæ. It is, however, probable as Baur and Case remark, that the Pelycosauria should be excluded from the Theromora and be placed in close relation with the Rhynchocephalia, to which order I have already referred provisionally one of the genera (Diopeus). That the authors agree with me that the Mammalia are descended from the Theromora is evident from their conclusion that the former may have been derived from the suborders Gomphodontia and Cynodontia, which are Theromora. They say, "These forms look very much like mammals and could possibly be ancestral to them." It is thus evident that Baur's term Sauromammalia, which he never defined, is a synonym of Theromora. In

[7] Transac. Amer. Philos. Soc., 1892, p. 25. Origin of the Fittest, 1887, p. 335–6, 346.

[8] Primary Factors Organ. Evolution, 1896, p. 88.

describing the conditions necessary to define the ancestors of the Mammalia, the authors remark: "The mammals have a single temporal (zygomatic) arch; the posterior nares are placed far behind, and are roofed over by the maxillary and palatine plates; the quadrate is completely coössified with the squamosal and quadratojugal; the occipital condyle is double, and the entepicondylar foramen is present in all the generalized forms. The ancestors of mammals must show the same conditions." It is to be inferred from the context that the authors mean that the Reptilian ancestors of the mammals must show these conditions. Important exception must be taken to these statements. The palate is extensively fissured in some Marsupialia, while it is closed in the Placodont suborder of the Theromora. The complete coössification of the quadrate is not to be looked for in a Reptilian ancestor, but its reduction must. Such I have shown to be the case in the Pelycosauria, in Diopeus and Naosaurus, and Seely has shown it to be still more reduced in the Cynodontia. The other characters are found in one or another of the Theromora. Hence I believe that the opinion that I advanced in 1885, that the Theromora are the ancestors of the Mammalia is the correct one.

Some interesting "asides" are to be found in foot-notes to this paper. The authors state correctly that I described two temporal arches in *Diopeus leptocephalus* and, therefore, placed it in the order Rhynchocephalia, and stated that the Pelycosauria have only one arch, which is homologous with the zygomatic arch of mammals. They then add "It is interesting to note that the latter result was reached by Cope (1884) on the identical specimen of (Diopeus) *Clepsydrops leptocephalus*." This statement, ascribing at the very least, gross carelessness to the author quoted, is throughout untrue. The ascription of a single temporal arch to the Pelycosauria was made by me in the original diagnosis of the suborder in 1880 (Proceeds. Amer. Philos. Soc., p. 38) four years previous to the discovery of the (*C.*) *D. leptocephalus* and in the description of the *C. natalis*, six years previously, in the statement "no quadratojugal arch." This means that the arch present, already described by me in *Clepsydrops natalis* in 1878 (Proceeds. Amer. Philos. Soc., p. 509) as a zygomatic arch, was still regarded by me as such.

In another foot-note the authors make the astonishing assertion that what I have called the columella auris in Diopeus is a rib. The skeleton of this specimen possesses ribs of the usual type, however, and neither in this genus nor in any other is there known a rib with a cup-shaped capitulum with a perforation of its peduncle. I have, moreover, figured a similar stapes in place in the allied genus Edaphosaurus (Transac. Amer. Philos. Soc., 1892, Pl. II, fig. 5a.) with perforation

below the disc. No free head was observed in the latter genus, but it may be concealed.

Dr. Alex. Götte, the distinguished Professor of Leipzic, gives a detailed account of his researches on the embryology of the caudal vertebrae of certain existing Lacertilia, with the view of demonstrating that my doctrine that the intercentra of the caudal vertebræ of the Reptilia are not the homologues of the intercentra of the dorsal series of other vertebrates, and that the conclusion that the vertebral bodies of the Anamnia are chiefly composed of intercentra, while those of the Amniota are centra, is incorrect. He commences by misunderstanding the (p. 376) ground of the doctrine he seeks to overthrow, a very common cause of unnecessary polemic. He says: "The alleged homology of all described intercentra depends exclusively on the assumption that the continuity of the chevron bones with the perichordal bone above them, indicates their genetic identity, so that the latter are an expansion of the bases of the former, or reversed, the chevron bones are processes of the perichordal bones. On the contrary, I can, on the basis of my observations on the development of the saurian vertebra, assert as a fact, that a genetic identity of the intercentra with their inferior arches does not exist, and that these parts originate rather as distinctly separate, as the superior arches and their vertebral bodies." No contradiction of these facts can be justly derived from my papers on the subject, and if I have used the word "continuity" in describing the relations of the chevron bones with the caudal intercentra, it has been in the sense of homological continuity, as in the case of the superior arches and the pleurocentra. That this is true is apparently proved by the facts of paleontology. The ground which is fundamental in this connection is the fact, that the elements which in the genus Cricotus do support the chevron bones and do not, or only in part support the neural arches, and which may be identified by their contracted superior long diameter, are continued all the way through the sacral, dorsal and cervical regions from the caudal, so that the homology may be directly traced. And secondly, because in some species of Cricotus the upper part of the intercentrum in the dorsal region is so pinched as to reduce the body to the form, as it has the position of a large reptilian intercentrum.

Dr. Götte denies the homology of the caudal and dorsal intercentra and of different intercentra with each other on the following grounds. First, the centra of vertebrata are not homologous bodies; second, the chevron bones in Batrachia are primitively distinct from the caudal

intercentra, and do not necessarily pertain to them; third, that in certain Lacertilia (e. g,, Anguis) the chevron bones are coössified with the centra, as is the case in the Batrachia Urodela; fourth, that the neural arches of the caudal vertebræ of Lacerta are partially divided on on each side by a fissure or foramen, which he regards as evidence that the vertebræ of reptiles consist of two original elements, that is, are produced by the fusion of the two bodies of the embolomerous type of column. His general conclusions are stated at the end of the paper as follows:

"(1). The construction of complete vertebræ with bodies and arches in the series of the Amiidæ, as in that of the Stegocephali and all living digitates begins in an embolomerous form, i. e., with double vertebræ to each segment. (2). The change of these double vertebræ into simple ones is accomplished by the fusion of the pairs after both vertebræ more or less, or especially the posterior one, have retrograded. (3). The rhachitomous vertebra is neither a primitive nor an independent appearance, but only a transitional stage in this change. (4). The principal significance of the embolomerous origin of the vertebra for the digitate vertebrates lies in the inheritance of certain remains of the double structure, the arches, transverse processes and ribs whose permanent forms are only to be understood on this ground."

As regards the question of the non-homology of the vertebral bodies, I believe that I have shown that they are for the most part not homologous as between the Anamnia and the Amniota, but that the homology of the contents of each of these divisions is shown by paleontologic evidence. It is also clear that many if not all of the vertebral bodies in the two great divisions in question must be homologous, otherwise we must have as many original ancestors of the vertebrata as there may be kinds of vertebral centra, a proposition which no one will be found to believe. In fact the embryology of forms of life of comparatively recent origin such as the Lacertilia, is apparently, from Göttes researches, as it should be supposed a priori, incompetent to explain the phylogeny of structures which received their definite completion in the paleozoic ages of time. *Owing to cenogeny, is is quite certain that structures may be directly related phylogenetically, which may appear to be in their present ontogeny not homologous.* This consideration applies to the supposed non-relation of the chevron bones to intercentra. This relation is universally demonstrated by paleontology, and better evidence than the changes of position in late forms such as occurs in Anguis (to which I have added Anniella) and the snakes, must be cited to invalidate it.

As regards the precedence of the rhachitomous over the embolomerous type of vertebral column, paleontologic evidence demonstrates that this was the history as regards Teleostomous fishes and Digitata (or Amniota), as Zittel has shown to have been the case in the former and I have shown as to the latter. Embolomerous forms do not come first in geologic time in these divisions, but later. I have not made any attempt to interpret with respect to this hypothesis, the structure of the vertebral column in the Selachii. They afford, however, no support to Prof. Götte's hypothesis, since it is probable that the Selachian vertebral column originated in a rhachitomous condition. In notochordal sharks, e. g., the Ichthyotomi, the primitive vertebræ are represented by centra above and intercentra below, as in the Teleostomi and Stegocephali. The superior segment supports the neural arch, and the inferior the hæmal arch. Götte's first proposition, that the embolomerous condition is the primitive one, is shown to be untrue as to the true fishes by the facts adduced by Zittel and others, since the primitive vertebræ of fishes described by these authors is rhachitomous and not embolomerous. Prof. Götte does not observe that his fig. 6 (text, p. 384) of Callopterus, represent rhachitomous caudal vertebræ, and dorsal vertebræ in which the centrum (pleurocentrum) is greatly reduced, so that the intercentrum becomes by far the larger part of the vertebral body. This is in exact accord with what is found in the Stegocephali, and is contributive evidence that the vertebral body in the Anamnia is intercentrum. That the body in the Amniota is centrum is abundantly proven by the characters of the Permian Pelycosauria.

In this study we have again an excellent illustration of the relative value of embryologic and paleontologic research in determining the homologies of parts and phylogenies of types. As to this Prof. Goette expresses himself thus (p. 377): "Since these relations can only be directly observed or completely known in living animals, and not in the fossil Stegocephali, so it is a self evident proposition that the unknown can only be explained by the known, the extinct by the living animals." This proposition must now, in view of the results of modern research be reversed so as to read as follows: *Since these relations can only be completely known or directly observed in fossil animals, and not in the embryonic history of living forms, it is a self evident proposition that the unknown can only be explained by the known, the living by the extinct animals.* Conceding the great value of embryology in the premises, it has now become fully evident that it can only be understood when interpreted by paleontology. An excellent illustration is the case of the embryology of the vertebræ of Amia, described by Hay and

Gadow, the former of which is discussed by Götte. The two researches only agree in discovering a much greater complexity in the ontogeny of these vertebræ than paleontology gives the least ground for supposing to have ever existed in the adult types of extinct Teleostomous fishes.

In his papers on primitive Edentata Dr. J. L. Wortman describes more fully than heretofore on new material, characters of the genus Psittacotherium Cope. He finds that it is armed with robust compressed claws, and that the foot is short and megatheroid in appearance. He interprets the dentition in a new way, and then homologizes with it the dentition of the genera Hemiganus and Ectoganus. The teeth formerly described as incisors in these genera he regards as canines. To these he adds Stylinodon Marsh, to form a family Stylinodontidæ. The genera Onychodectes and Conoryctes Cope he places in a family Conoryctidæ. Both he combines into a suborder Ganodonta of the order Edentata.

Whether the interpretation of the dentition of Psittacotherium and Ectoganus is correct or not depends on the interpretation of the same parts in Calamodon. Some doubt must still remain as to this point, a doubt which I have always felt. It is certainly not unlikely that Dr. Wortman's interpretation may turn out to be correct, and if true, a clearing up of the subject of the relation of these forms to the Tillodonta of Marsh will result. Accepting his view as correct, we have then a group having strong claims to being regarded as ancestral to the Edentata. This position I maintained as long ago as 1875, when (in the Report of the U. S. Geol. Geogr. Surv. W. of 100th mer., Vol. IV) I included some of these forms (Ectoganus and Calamodon) in a suborder Tæniodonta, and suggesting its ancestral relation to the Edentata. I have not pressed this view recently for the reason above referred to. The name was, however, given, and it was applied to a group so far equivalent to Wortman's name Ganodonta, that as matter of taxonomic rule it cannot well be displaced. His reasons for rejecting the name are that I referred two of the genera (Conoryctes and Onychodectes) to the Creodonta, which I still do; that I failed to recognize the affinities between Calamodon and Stylinodon, which, however, I always have done so far as the imperfect description of Marsh would permit. Thus, in my Synopsis of Families of Vertebrata, AMERICAN NATURALIST, October, 1889, I place in the suborder Tæniodonta, the two families Ectoganidae and Stylinodontidae. Wortman concludes also that the name must be rejected because founded in error. If this be true, it is

no reason for the rejection; on the same ground nearly every name in Biology above the specific would have to be rejected.

Although Dr. Wortman makes excellent use of the material at his disposal, and throws much light on the characters of some of the genera, the evidence for the reference of the Tæniodonta to the order Edentata, must be considered as yet very obscure. But the reference to that order of Conoryctes and Onychodectes is still more difficult, if not impossible. If proper, a new definition of the Edentata must be forthcoming. I am still of the opinion that the best provisional place for these two genera is in the Creodonta, next the Tæniodonta, to which they have probable affinities. The position of Dr. Wortman is based on the scientifically untenable assumption that because forms probably stand in the relation of ancestor and descendent they must therefore belong to the same genus, family, order, etc. He goes so far as to place Esthonyx in the Tillodonta, to which it is probably ancestral, although it does not possess the essential character of that order or suborder—incisors growing from persistent pulps. For equally valid reasons all the genera of a phyletic line might be regarded as a single genus. This kind of formulation casts to the winds all taxonomy, and the effect of it is seen in this instance in Dr. Wortman's failure to define the order Edentata. It was the consideration of such forms as Conoryctes and Onychodectes with Esthonyx and the Tæniodonta and certain Insectivora, that led me to propose the comprehensive order of Bunotheria, which is the source of all the Unguiculate orders of later time.

Professor Marsh's article is a much needed description of his genus Stylinodon, of which he has obtained some important parts of the skeleton. It looks more like an Edentate than any of the other Tæniodonta, with which I placed it in 1889. The figures which he gives, will prove valuable to paleontologists, but more light will be necessary before its relation to the Edentata can be determined. Prof. Marsh cannot let the opportunity pass without proposing a new subordinal name, "Stylinodontia," which he does not characterize, although there are already two other names in the field before him, one of which, Tæniodonta, was proposed and defined twenty-one years ago. The rambling discussion as to the origin of the Edentata which closes this paper adds nothing to our knowledge of the subject, especially as it includes the names of genera which he has never defined, and which are so far unknown to science.—E. D. Cope.